

that are similar in their features to those here taken into consideration. If we summarize the deductions above we may say:

I. The lows that move with accelerating velocity are in general characterized by the greater advance of the front of the dead line on the right side of the track of the low than on the left; the track of the center of rise keeps either on the right side or crosses to that side from the left at an acute, a right, and even an obtuse angle.

II. The lows that move with decreasing velocity are characterized by having the front of the dead line perpendicular to the track of the low or with a greater advance on the left side, the track of the center of rise keeping in most cases either on the track of the low or very close to it, usually being on the left side or sometimes even crossing from the right to the left.

In a similar way we could study the dead line in the front of the areas of fall and the movements of the centers of the areas of fall as I have done above. If we look through a series of maps of the character described above we find that the centers of the areas of fall generally precede more or less closely the centers of the advancing lows and are no doubt fairly good indicators as to how the lows will move in the next twelve hours, but their relations to the lows on account of their proximity appealed to me less than the characteristics of the areas of rise and their centers.

* * * * *

In presenting this short study on areas of rise and fall I can not close without an appeal to those who *see and read* better than the writer of this article the daily maps showing the changes of meteorological elements, and to whom, perhaps, part of what has here been said does not appear as something new. The drawing of this kind of charts, which has been done at Washington for a long series of years, and their use for forecasting in the hands of expert forecasters have certainly given rise to many empirical rules about the relation of the "dead line" to the areas of fall and rise, but so far as I know these rules have never been presented for publication to be used by others also and to be interpreted by those who have gained a deeper knowledge of the dynamical nature of cyclones and anti-cyclones. So with the retirement of a forecaster all of his empirical rules on weather forecasting are lost without becoming the property of practical meteorology. How, then, can one expect an advance in forecasting?

These empirical rules should become the general property of meteorology and should be published, not only for the mutual help of forecasters, but also in the interest of meteorology as a science, even if the forecaster himself can not give a physical explanation of the rules found and used by him.

We draw and write about types of weather; why should we not in a similar way treat pressure, temperature, and other charts? The practical forecaster looks at the weather map with a different eye from the scientific meteorologist, and probably many of the empirical rules of the forecaster can be given a physical explanation by a scientific meteorologist. On the other hand a physical law may receive a practical application in the hands of an expert forecaster.

As an example of this I call attention to the paper of Mr. Bowie, published in the report of the Peoria Convention of Weather Bureau Officials,⁶ where he expounds a new method of determining the direction and velocity of the movement of a storm and shows that the direction and the amount of movement is a resultant of two forces, namely:

(1) The component of the eastward drift, (2) the resultant of the pressure "exerted on the storm center" (to use his own words) from all directions. He tries to find some simple,

geometrical scheme for the expression of these two forces and thus to determine the track of the low.

I know not whether it was known to Mr. Bowie or not, but in reality his text does not say anything more than what is known in meteorology as Kœppen's law of the movements of cyclones, stripped of its physical and dynamic expression, and put in a simplified form applicable to weather forecasting.

That the application of the pressure change charts for weather forecasting is something new in Europe seems to be indicated by the lines of the paper of Professor Ekholm, who either intentionally omits any note on the use of the charts in the United States or else does not know about it. To prove that those empirical rules would be of great interest to many who have read Ekholm's paper just mentioned and have started on this study, I may refer to a note by Mr. Friesenhof (*Meteorologische Zeitschrift*, May, 1905), who grows quite enthusiastic over the advice of Professor Ekholm.

I therefore wish to finish with the remark that the empirical rules, based on long years of use of these charts in question, should form the subject of many articles in the *MONTHLY WEATHER REVIEW* for the benefit of practical meteorology, which is to-day based *only* on them; their physical interpretation will give us a firmer basis for understanding the weather.

GLAISHER'S FACTORS AND FERREL'S PSYCHROMETRIC FORMULA.

By Prof. C. F. MARVIN. Dated June 5, 1906.

An article in the Quarterly Journal of the Royal Meteorological Society, Vol. XXXII, No. 173, pages 35-45, January, 1906, by J. R. Sutton, gives a discussion of the differences between dew points and humidities as deduced from readings of the wet-bulb and dry-bulb thermometers by the use of Glaisher's tables of factors of reduction and Ferrel's psychrometric formula and tables.

The writer calls attention to large and striking disagreements in the results derived from these two tables under certain exceptional or extreme conditions, but he afterwards makes this significant statement, page 40:

After all, there is not so much difference as one might expect between the monthly means * * * computed respectively by means of the Greenwich factors * * * and by Ferrel's psychrometric formula.

In illustration of this the mean hourly values for the months of March and November, 1904, respectively the wettest and driest months, are compared, from which it appears that the mean dew-point for March, by Ferrel, was 0.5° higher than by Glaisher's factors; whereas, for the dry month of November the mean dew-point by Ferrel was 2.6° lower than by Glaisher. In all these observations the wet bulb was subjected to only such ventilation as resulted from the motion of the natural wind. The kind of screen or thermometer shelter employed is not mentioned. In order to make the comparisons the more fair, however, Sutton has applied an empirical correction to the wet-bulb readings, based on the wind velocity observed 40 feet above ground for half an hour before each reading.

In any discussion of the relations and discordances between psychrometric formulæ, it is highly important to keep clearly in mind: 1st. That the psychrometric formulæ commonly employed, even at the best, only approximately express the law of relation between the wet-bulb temperature and the moisture content of the air. 2d. That the numerical coefficients of these formulæ are necessarily computed from certain tests observations which, unfortunately, are always comprised between far too narrow and restricted limits. It is a comparatively easy matter, for example, to make many thousands of simultaneous dew-point and wet-bulb determinations and compute therefrom the coefficients of a psychrometric formula; of these observations, however, relatively a very few will com-

⁶For second edition see *Monthly Weather Review*, February, 1906, Vol. XXXIV, p. 61.—Ed.

prise properly combined extreme conditions of temperature, pressure, and dryness. The result will be that a given psychrometric equation may possibly be made to fit the ordinary range of atmospheric conditions very well, whereas the formula is very likely to be weak and unreliable in its application to extremes of either temperature, pressure, or dryness.

It seems to the writer to be well recognized by all experienced investigators that approximate formulæ, even when supposed to rest upon a more or less rational basis, can be depended upon only within the range embraced by the observations from which the coefficients of the formulæ were deduced. Extrapolation, to any considerable extent outside this range, is almost certain to involve more or less serious errors.

The foregoing considerations afford, I think, an entirely satisfactory explanation of the very radical differences between Glaisher and Ferrel pointed out by Sutton. Thus, for the observed air temperature 50° F., wet-bulb temperature 34° F., we get the dew-point by Glaisher +17° F., but by Ferrel -1° F., etc.

It is quite likely that this example represents conditions quite outside of any of the actual test observations employed by either Ferrel or Glaisher.

These same considerations satisfactorily explain the fairly close agreement between the observations under ordinary conditions when reduced by the same two authorities, as illustrated in the computations of the hourly data for March and November, even though these be relatively extreme conditions. It is interesting to plot the March and November results, as shown in fig. 1.



FIG. 1.—Differences between the dew-points computed by Glaisher's and by Ferrel's methods.

During March the air is relatively moist, except in the middle of the day, and the dew-points are almost perfectly constant. We must expect the results for such a month to agree closely simply because the coefficients of the psychrometric formulæ have been fitted to just such observations. It is fair to suppose that the fundamental data employed by both

Ferrel and Glaisher¹ must agree pretty closely, especially for the ordinary atmospheric conditions, whereas, under more unusual conditions, as, for example, under the relatively dry conditions of the month of November, the differences are noticeably greater, although still not seriously discordant except in the middle of the day, when the humidities run down to nearly 20 per cent. As Sutton points out, Glaisher's equation admits of very great depressions of the wet bulb, with low humidities, whereas by Ferrel the humidity rapidly runs down to zero as the depression of the wet bulb increases. We think it is quite likely that if Glaisher's original data could be critically examined it might be found either that observations under extreme dryness are few in number or that the adopted factors do not fit these observations very well.

It would seem that the interpretation of Ferrel's formula in empty space requires a different statement from that given by Sutton who concludes that the wet-bulb reading would fall to absolute zero. If the idea of "empty space" involves the necessity that the vapor pressure e is zero, as well as the air pressure P , we must conclude that no water can be present on the wet bulb, at least no vapor may be given off, and hence no cooling could ensue. In fact, the argument amounts to an attempt to fit the formula to irrational conditions, and the result is indeterminate. A more rational basis of analysis is to assume that the pressure in such a space is only that due to the vapor present, that is, $P = e$, whence

$$e = \frac{e'}{1 + A(t - t') \left(1 + \frac{t' - 32}{B} \right)}$$

which is an entirely rational result.

The coefficient A , however, is a function of the rate of diffusion of vapor from the wet bulb and also of the coefficients of conduction and radiation of heat.² It is not to be supposed, therefore, that the same numerical value of A , found to suit ordinary atmospheric conditions, would suit the extreme conditions of the present assumption, under which the conduction of heat to the bulb and the diffusion of vapor from it are so radically different.

So much has been written demonstrating the necessity of using some degree of uniform ventilation for the wet-bulb thermometer, that we know for a certainty that unsatisfactory ventilation can only result in accumulating observations that are affected by many irregular errors. The plan worked up by Sutton, of using a series of corrections to the wet bulb depending upon the wind movement, is very tedious as well as only approximate. The wind movement ought to be measured at the wet bulb, not at "40 feet above ground". Very few observers could be expected to deduce properly such a series of local corrections, and corrections found suitable at one observatory could hardly be safely employed elsewhere, unless the conditions of exposure were substantially identical. Some form of aspiration fan, such as employed by Assmann in certain forms of recording apparatus, is necessary for continuous observations.

Glaisher's factors do not appear to be founded on any physical basis, whereas Ferrel's equation expresses practically all that is at present known respecting the psychrometric law as

¹ The observations employed by Ferrel were specially made for the purpose by Professors Hazen and Marvin, and comprised as great a range of conditions as possible. These are fully given in the Annual Report of the Chief Signal Officer for 1886, p. 233, et seq.

Glaisher states that his factors are based on a large number of dew-point observations, but the writer is not aware that these have been published.

² Recent Advances in Meteorology. Ferrel. Ann. Rept., C. S. O., 1885. Part 2, p. 387. See also Meteorological Apparatus and Methods. Abbe. Ann. Rep., C. S. O., 1887. Part 2, p. 371.

dependent upon the diffusion of vapor from a wet bulb and its gain or loss of heat by conduction, radiation, etc.

While such "factors", carefully determined, may give very satisfactory results over a limited range of conditions, yet when we are called upon to choose between distinctly discordant results under more or less unusual conditions, such, for example, as those given by Sutton for November, it would seem the argument is entirely in favor of Ferrel.

The great source of unreliability in existing psychrometric tables is the insufficient or unsuitable experimental basis from which to compute the psychrometric constants. Many thousands of observations have been made and used for this purpose, but when they are subjected to critical analysis thousands of them are found to fall within a narrow range of moisture and temperature conditions, while very few cover those extreme combinations of temperature, wet-bulb depression, and pressure that are required in order to ascertain what values of the psychrometric constants are suitable for the wide range of conditions that is usually embraced in such tables.

A special source of error in psychrometric tables lies in the determination of the fundamental dew-point by means of the condensation hygrometer or dew-point apparatus. The instruments as delivered by the manufacturers, whatever may be their name or form, are all fundamentally bad, and physically objectionable for the following reasons: (1) There is an unnecessary mass of material to be cooled to the dew-point; (2) an adequate supply of refrigerant is not provided for; (3) the idea that the conduction of heat to the chilled surface should be cut off as far as possible, appears never to be taken into consideration by the maker, (this defect is, however, a fruitful source of unequal temperatures and consequent errors, and causes a waste of refrigerant and effort); (4) the free space provided for the bubbling of the ether is always entirely inadequate in those instruments in which this refrigerant is employed.

When, in 1885, the writer undertook the dew-point determinations for the Weather Bureau he improvised the apparatus shown in fig. 2. The thin silver cup *a*, was taken from a so-called Regnault's dew-point apparatus. This was cemented upon an ordinary chemist's test tube, cut off at the lower end. A nicely fitting cork stopper carried a sensitive thermometer in a center opening, and two small lateral tubes of hard rubber. One of these, *b*, extended quite to the bottom of the silver cup, the other simply passed through the cork and a long rubber tube attached to its outside end served to carry the fumes of the refrigerant entirely clear of the apparatus.

A wooden, or cork-faced clamp supported the glass tube at a point well above the cup *a*. We realize in this construction (1) the minimum mass to be cooled; (2) the maximum relative supply of refrigerant; (3) a very satisfactory heat insulation; (4) a generous free space for the ebullition of the ether the cold fumes of which, as they ascend to escape, completely envelop the stem of the thermometer so that the bulb and the mercurial column have nearly if not quite the same temperature.

To secure accurate dew-point determinations by means of such an instrument the cup must first be highly polished and cleaned. For this purpose the writer has used a little oil and "putz-pomade." The rubbing motion is always longitudinally applied with the hand only. To clean off the grease the hands are first thoroughly washed in a copious lather of soap, discolored at first but finally snow-white, whereupon the cup and tube are cleansed in a similar lather worked with the hands. The clean soap suds are finally rinsed off in an abundant water supply and the apparatus withdrawn from the clean water. The glass surface will remain wet, but the silver will emerge from the water *absolutely dry and clean*. Possibly a single drop of water may adhere to the extreme bottom of the cup and may be removed by a touch of a towel which should

be used to dry the glass tube, but the surface of the metal cup must not be touched for any purpose, as the surface will be soiled and the deposition of the dew will be modified.

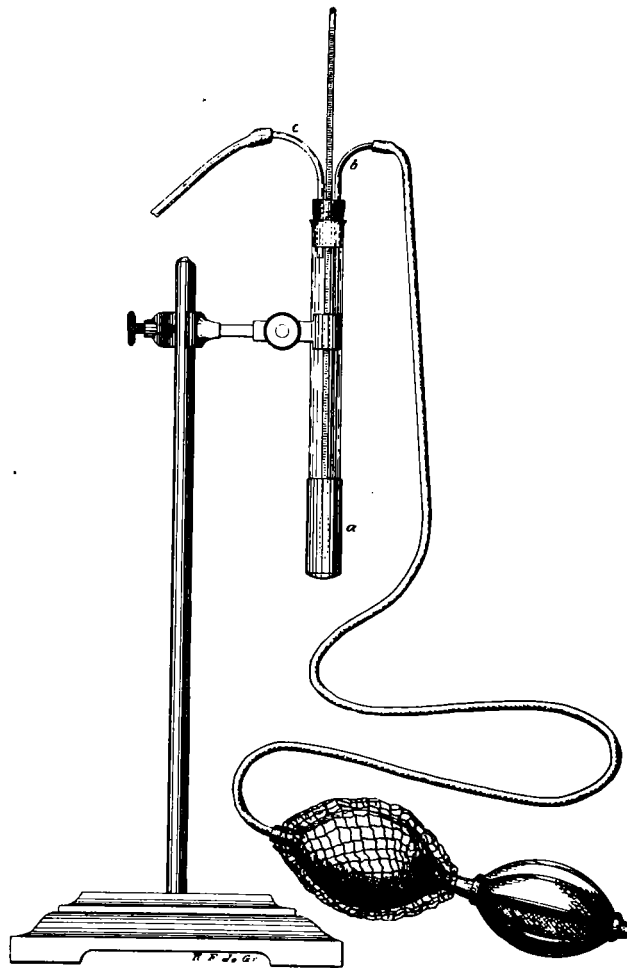


FIG. 2.—Dew-point apparatus.

This operation of cleansing the cup is difficult to describe in detail, but it is easy to perform and it must be employed every few days if the instrument is used regularly. Certain other precautions are necessary. (1) The place of observation must be selected with a view to securing (*a*) perfectly free ventilation, without any appreciable drafts, (*b*) the proper illumination. This is a troublesome detail. We must be able to see the surface of the cup. Its highly polished surface causes it to reflect any strong source of illumination. This must be cut off, as far as possible, as it causes us to perceive the strong illumination and prevents us from seeing the actual reflecting surface of the cup, hence we fail to see the minute incipient deposition of dew. In general, the instrument requires a well-lighted location in an inclosure with darkened walls which latter give a black surface as reflected from the surface of the cup. That is to say, by reflection the cup itself seems to be black and not of silvery whiteness. The slightest deposit of dew on such a cup can be discerned.

(2) The observer must exhale through a long tube. A provision for this purpose enables him to proceed deliberately with his experiments and permits of the most critical inspection of the surface of the hygrometer at short range with the minimum prejudicial influence due to his presence and without the discomfort of any restraint upon his natural respiration.

(3) The deposition of the dew must be produced in the fol-

lowing way: The tube is kept filled with refrigerant³ to slightly above the level of the silver cup. The bulb of the thermometer is kept slightly above the mid-section so as not to be too near the air entering at the bottom. A preliminary cooling shows the approximate temperature of the dew-point. The cup is allowed to warm up a trifle, and the cooling is then made to proceed very slowly until the slightest possible deposition of dew appears, when the temperature is noted. The cup is again permitted to warm up a little by checking the cooling, but still maintaining a slight flow of air to prevent any too sharp changes of temperature and to keep the liquid thoroughly stirred. A second deposit of dew is formed and its temperature noted. In the meantime a gentle circulation of air in the vicinity of the hygrometer is maintained by means of a common fan. Owing to the small mass and good heat insulation of the instrument the temperature of the cup can be controlled admirably so as to cause dew to deposit and vanish at will and the temperature maintained nearly constant just at the dew-point for several minutes, thus affording several determinations.

If the observation is correctly conducted the difference between the temperatures at which the dew forms and vanishes, respectively, is too minute to be discerned by the thermometer in the liquid; at the same time there is generally an appreciable difference in successive determinations, just as there is a difference in the successive values of the wet and dry-bulb temperatures.

In the Colorado observations made by the writer on and near Pikes Peak, his practise was to make five readings of the dew-point, followed by ten readings on the whirled psychrometer; followed again by five dew-point readings. The sums of these respectively, with the decimal points properly placed, gave the numerical result of one experiment. Nearly 1000 experiments of this sort are incorporated in Ferrel's psychrometric tables as now used by the Weather Bureau. These tables are based on Ferrel's rigorous formula (20), page 249, Annual Report of the Chief Signal Officer, 1886, viz,

$$p = p_1 - 0.000660 P (\tau - \tau_1) (1 + 0.00115 \tau_1) \text{ centigrade,}$$

or,

$$p = p_1 - 0.000367 P (\tau - \tau_1) \left(1 + \frac{1}{1571} (\tau_1 - 32) \right) \text{ Fahrenheit,}$$

instead of his approximate expression

$$p = p_1 - 0.000660 P (\tau - \tau_1) \left(1 + 0.00115 (\tau - \tau_1) \right) \text{ centigrade.}$$

IMPROVEMENTS IN SEISMOGRAPHS WITH MECHANICAL REGISTRATION.

By C. F. MARVIN, Professor of Meteorology. Dated May, 1906.

About a year ago the writer began a series of experiments with a view to making improvements in seismographs such as are employed for the absolute or quantitative measurement and registration of the motions of the ground during earthquakes, especially in those instruments in which the registration is effected mechanically by a stylus tracing its record upon smoked paper.

The work has progressed rather slowly and with many interruptions incident to the discharge of other engrossing duties, and much still remains to be accomplished before we can feel assured that our seismograms are faithful reproductions in detail and to scale of the actual motions executed by the piers that support the instruments. Certain improvements, however, have now been tested so fully and with such satisfactory results, and the devices may be so easily attached to practically any form of seismograph with mechanical regis-

tration, as to warrant this short account of the work thus far accomplished.

Early in 1903 a set of the Bosch-Omori horizontal pendulums, with mechanical registration, were procured for the Weather Bureau at Washington, D. C., and one of these instruments at once installed. Owing to the lack of a suitable place of exposure, the other pendulum was not set in operation until July, 1905, and then only in a temporary location. More recently, however, both instruments have been reinstalled in a new location under conditions which are quite satisfactory, although still subject in a slight degree to the influences due to the immediate environment. The improvements about to be described have been applied to these instruments, one of which, in its original form, is shown in figs. 1 and 2, and was fully described in the MONTHLY WEATHER REVIEW for June, 1903, Vol. XXXI, pp. 271-275.

The steady mass *C* is considered generally to remain at rest during the earthquake. The forked short end of the lever *L*, fig. 2, engages the delicately pivoted pin *f* forming part of the steady mass *C*. This point of the lever therefore remains at rest. The motion of the ground is communicated to the lever at its pivot *d*, carried in the massive yoke piece *G*, which in turn is firmly secured to the ground or pier. It results from this disposition that lateral relatively quick vibratory motions of the ground will be registered in a magnified fashion as a sinusoidal curve by the long end of the lever *L* tipped with the stylus as shown enlarged at *S*, fig. 2. Assuming the reader to be already familiar with the main features of the operation of seismographs we shall proceed at once to the discussion of one of the chief sources of errors in these instruments, namely, friction. The steady mass of the seismograph, if slightly disturbed, will in general be very soon brought to rest by the unavoidable friction in the joints of its mechanisms. On the other hand, during an earthquake this same friction tends likewise to set the mass in motion. Indeed, very small earthquakes often fail to be recorded at all because the friction at the joints of the recording stylus in scratching off the soot literally forces the steady mass to move exactly with the ground, so that no record is obtained. The elimination of friction to the utmost degree is therefore of the greatest importance. Another source of error in the Bosch-Omori instruments that seemed to cause the loss of records of the very small vibrations was in the use of the forked end of the lever *L* and the pin *f*. The width of the forked opening could never be adjusted to fit the pin properly. There was always a little lost motion at this joint, or when the pin fitted exactly, the friction was excessive. In setting about to improve our instruments in this respect, one side of the forked arm of the lever was cut away entirely, and a very delicate hair spring of a watch was fastened to the pivot of the lever *L* so as to cause the short end of the lever to bear very gently against the pin *f* of the steady mass. The tension of the hair spring was adjusted so as to be just enough to easily scratch the soot on the record sheet. By the new arrangement the stylus is moved in one direction only by the force of the spring, whereas motion in the opposite direction is caused by the direct push of the pin *f*. This alteration led at once to three important discoveries.

(1) *Increased sensitiveness to short-period tremors.*—The instrument was now found to record minute short-period vibrations of the ground and pier to which previously it had never been in the slightest degree sensitive. The rapid driving of vehicles along an asphalt street about 90 feet distant caused a perceptible thickening of the trace, whereas a large record was made every time a wagon passed over a cobble paved private way only about 30 feet from the pier upon which the pendulum had been temporarily installed for the purpose of this study.

(2) *Improved method of marking time.*—The second valuable

³ The writer finds Squibb's sulphuric ether about the best.